

June 17, 1948.

Dr. Lois Dickinson,  
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England.

Dear Dr. Dickinson,

Having been led to it by Werner Braun's recent review on bacterial dissociation, I have just now read your paper "The influence of substrate on the variation of *Br. bronchiseptica*" which appeared three years ago in the Journal of Pathology and Bacteriology. If, as you would seem to have concluded, agents can be found which have an anti-mutagenic effect in an otherwise "normal" environment, it would be very remarkable indeed.

Your experiments undoubtedly tend to this conclusion. Your "reconstruction experiments", in which small inocula of the V form were added to cultures predominantly W are particularly impressive. For some time now, I have been belaboring the necessity of such experiments as controls on mutagenic activity.

If your conclusions were not so remarkable, I would not raise the issue, but it seems particularly important in such a study as this to eliminate every possible source of error. On p. 291, you report that "In chloride-free medium mixtures containing up to one loopful of V did not reveal V colonies after 20 subcultures...." This would seem to mark one loopful as a critical concentration of V cells for their establishment in a culture. Later, you comment that "In maleicate medium ... the weakest mixture tested contained one loopful of V and this rapidly became pure V." From this you would conclude that the maleicate did not influence the selection dynamics involved in the establishment of the V form. I would like to suggest the importance of extending your observations to increasingly smaller inocula of the V form in mixed culture with W, in order to show that maleicate does not simply ~~reverse~~ the adverse selection which V seems to suffer in mixed cultures in chloride-free ~~medium~~ ~~maintaining~~ -free medium.

Your comments on this discussion would be appreciated, as would a reprint of the paper.

Yours sincerely,

Joshua Lederberg  
Assistant Professor of Genetics.